

Volume 7 Number 4/October 1957

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The Psychological Record

a quarterly journal in theoretical
and experimental psychology

Published at the UNIVERSITY OF WICHITA, Wichita, Kansas

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The Psychological Record is a non-profit publication. It is published quarterly, in January, April, July, and October, at the University of Wichita, Wichita, Kansas. Subscription price is \$3.00 a year. For students the price is \$1.50 a year.

With the permission of the Principia Press, Inc. *The Psychological Record* is a continuation of the journal formerly published under this title.

Publication of *The Psychological Record* was resumed in January, 1956 with the express intention of furthering the developing interrelationship of those psychologists interested in interbehavioral theory. The journal no longer serves this purpose and is now not committed to a particular theoretical point of view.

As presently organized *The Psychological Record* publishes both theoretical and experimental articles, commentary on current developments in psychology, and descriptions of research planned or in progress. The journal is designed to serve a critical function in psychology. It therefore favors the publication of papers that develop new approaches to the study of behavior and new methodologies, and which undertake critiques of existing approaches and methods.

Articles should be prepared according to the form suggested for A. P. A. publications (*Psychol. Bull.*, 1952, 49, 389-449) and submitted in duplicate to the editor: Dr. Paul Swartz, Department of Psychology, University of Wichita, Wichita, Kansas. The author cost per page is \$3.00. There is an additional author charge for cuts and special composition. Reprints are available at cost.

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FIRE-WALKING AND RELATED BEHAVIORS

MAYNE REID COE, JR.

311 West 14th St., Riviera Beach, Florida

Editor's note: In the August 1957 issue of *True* magazine Mayne Reid Coe, Jr. described his early attempts at fire-walking and related behaviors. Shortly after this article appeared, the editor of *The Psychological Record* visited Coe and observed him perform three of the behaviors reported in the following paper. These were: (a) touching a red-hot iron bar with a finger, (b) stamping a red-hot iron bar out of shape with a heel, and (c) splashing a puddle of molten iron with several fingers. Coe's contacts with the hot objects were of very brief duration and no burns resulted. The editor is convinced of the validity of these acts, particularly since they were repeated in his presence by a third party who had never met Coe and who was a disinterested and completely reliable subject. A fourth behavior reported by Coe, covering his hands with carbon black by placing them in candle flames without incurring any burn, has been successfully repeated by an associate editor of this journal. Coe's fire-walk has been filmed by Twentieth Century Fox Movietone News.

Mayne Reid Coe, Jr. is a chemist, attempting to find a naturalistic explanation for behaviors long regarded as "old wives' tales" by scientists and as supernatural by laymen. We think that his work deserves the widest possible reading among professional psychologists, not so much for the validity of his interpretations but merely to establish the existence of the kind of data he has stumbled upon. It is for this reason that we have asked him to write the article which follows immediately below.

To most people, scientists included, the ability of certain persons to resist the burns ordinarily attendant upon contact with incandescent objects, is completely incomprehensible. They will invariably declare, upon hearing of such events, that they are impossible, that some trick is involved, such as treating the skin with chemicals, or that the actors involved resort to mass hypnotism. Let us examine the evidence and see what we find.

First we must establish the fact that such events actually occur. Below, we present the evidence, much of it anecdotal in nature, which comes to us in abundance from widely divergent sources. This is supplemented by a description of the author's own successful attempts at fire-walking and related behaviors.

HISTORY

Mention is made in the *Old Testament* of Shadrach, Meshach, and Abednego walking in the fiery furnace and emerging from the ordeal unscathed. Although the account is perhaps somewhat embellished in the telling, it takes on meaning in view of events that followed. Thus, from the early centuries of the Christian era down almost to modern times, there have been numerous reports of the amazing invulnerability of certain saints to fire, burning coals and red-hot iron. From the middle ages come tales of trial by fire, in which innocence was established if the party involved could run barefoot over red-hot ploughshares without being burned, or could hold a piece of red-hot iron in his hand without injury. Incredible? Yes! But as evidence piles up, we look back at those ancient times and wonder if past peoples did not possess knowledge lost to modern man.

Let us begin with the saints. Who in this marvelous scientific age of ours ever bothers to read of their exploits? And who, doing so, believes? In our day and age, to do so makes one suspect. In fact, if you were to go to a psychiatrist, the product of our highest institutions of learning, and say, "I really believe that saints handled red-hot iron without being burned and that they were capable of other amazing acts, such as levitation, as well," he would certainly suspect you of psychopathy. And for this he could not be blamed. But the testimony of apparently faultless witnesses surely has some bearing here. Such reports were not considered as evidence in raising a man to sainthood unless the sources were unimpeachable. Regardless of the remoteness of the times, and other considerations as well, this evidence cannot be overlooked.

Butler's *Lives of the Saints* contains numerous examples of saints handling burning coals, cleaning out red-hot embers in stoves with bare hands, etc. M. Oliver Leroy and Herbert Thurston, S. J., have recounted the most interesting of these instances, and from their writings I will borrow a few (3). Leroy and Thurston themselves, however, freely admit that many of these amazing happenings may well have been embellished by the saints' over-enthusiastic hagiographers.

The first instance I wish to relate, that, in the light of later happenings, could have occurred, is recounted by St. Gregory of Tours. Lacking sufficient corroborative evidence, we need not be quick to accept it as having actually taken place. However, it could easily be duplicated, as will be evident after further matters are discussed.

During the sixth century, a Christian, in the course of a dispute with an Arian concerning the truth or falsity of the doctrine he upheld, threw his gold ring into a blazing fire and dared his opponent to recover it. The Arian feared to do so. The other thereupon reached into the fire, and searching the embers with his hand, found the red-hot ring and held it in his palm without the slightest injury.

The next instance, which parallels the feats of contemporary fire-walkers, concerns the behavior of St. Peter Igneus. In the year 1062 a certain party managed to be elected Bishop of Florence by resorting to bribery. As a result, the people, knowing his reputation, were greatly incensed and uprisings occurred. In order to quiet them, St. John Gualbert, Abbot of Vallombrosa, decided to appeal to "the judgment of heaven" by resorting to trial by fire. Accordingly, he asked Peter Aldobrandini, a monk later given the title of St. Peter Igneus, to undergo an ordeal by fire. If he should emerge unharmed, the bishop would be proved to be a fraud.

Two long piles of wood, separated by a path just wide enough to walk between them, were set on fire. These were allowed to burn until the passage-way between them was completely strewn with glowing red coals. At this point, Peter walked barefooted along the entire length of the passage-way between the two piles. No harm came to him whatsoever. As a result, the bishop was removed.

Next, consider the feat of the Augustinian hermit, Blessed Giovanni Buono, who apparently duplicated the behavior of Peter Igneus. Giovanni's feat is of further interest because it was offered in the cause of his beatification in 1251 and was corroborated by several witnesses.

It seems that the brethren were discoursing around a great fire on a cold winter day and that Giovanni was lecturing them on the absolute necessity of holding to their faith regardless of hardships, telling them that they should fear nothing, neither great cold nor extreme heat, nor sufferings, etc. God, he argued, would always aid them when they needed him most.

And saying this (the witness went on) John suddenly rose up and stepping into the fire he began to shuffle the embers about with his feet just as if they were water, and there he remained standing for as long a time as it would take to say the Miserere half way through. Then, quitting the fire, he went back to his cell and sent for Brother Matthew, as well as for this deponent and two other brothers of the same Order whose names he has forgotten. He told them that they must be the friends of God and love Him dearly; but since this deponent was convinced that Brother John Buono had suffered hurt from the said embers, he purposely came close to the same John that he might better examine and observe whether any damage had been done to his feet or his legs or his tunic, but, though he scrutinized them narrowly, he saw no trace of burning or of any injury.

Another unusual occurrence is related by Mother Francis Raphael. This is the story of St. Catherine of Siena.

Another day, being engaged in the kitchen according to her custom, she sat down by the fire and began to turn the spit; as she did so she was rapt in ecstasy . . . When supper was ended Lisa returned to Catherine, intending to watch by her until she should recover consciousness. On re-entering the kitchen, however, she was terrified to find that Catherine had fallen forwards, and was lying with her body on the burning coals. The fire was large and fierce, for an unusual quantity of wood was always kept burning in the house for the sake of preparing the dyes. "Alas!" cried Lisa, "Catherine is all burnt"; and so saying she ran and drew her out of the smoking embers, but found to her wonder that she had received no injury either in her person, or even her clothes, on which the "smell of fire had not passed." "And yet," says her old English biographer, "it was a great fire and she a long time in it."

Of particular interest is the behavior of St. Francis of Paula. The following is taken from material presented during his canonization process. It is recounted by a witness to the incident, Bernardinus de Raymundo.

It seems that Francis was present at a smithy while a horse was being shod. A bar of red-hot iron was left over, and Francis inquired of the smith if there was enough to do a similar job for him. As the man indicated the glowing red bar, Francis picked it up in his hands. Horrified, the others shouted to him that he would be burned. Francis replied, "By your leave, I am just holding it to warm myself."

Other accounts of St. Francis report that he often carried glowing coals in his hands, even offering them to others to hold, apparently indicating that he was extending immunity to them. He had been known to enter red hot lime kilns to repair them and had even placed his bare feet over the flames leaking from the stacks of charcoal burners, until earth could be brought to cover the openings.

Such instances of the behavior of saints are very numerous, and it would be pointless to relate any additional ones. Let us turn instead to a consideration of several cases in which there are no religious aspects in the behavior involved.

The first is mentioned by M. Leroy and is taken from the famous *Evelyn's Diary*. It describes the well known fire-eater Richardson, during a visit at Leicester House. There, in front of an enthralled audience, he ate burning sulfur on red, glowing coals, swallowing the whole mess after chewing it. Then, melting a thin glass in the fire-place, he would bite off a piece of it at a time, swallowing these pieces after chewing them. Another feat was to take a live coal on his extended tongue and, placing an oyster on it, have the coal blown on with a bellows until the oyster was cooked. His feats are thoroughly described in the journals of the time, both in England and on the continent. The above stunts and ones similar in nature were performed often, to the intense enjoyment of royalty and other prominent people (3).

The next case concerns Signora Girardelli of Italy, who toured England in the early nineteenth century, performing at fairs. Among other behaviors, she would heat an iron shovel to redness and pass it over her body without being burned. Then, she would lick it with her tongue while it was red hot. The hiss of the contact was clearly audible. Again, she would heat a shovel to redness and bend it by stamping it with her bare foot. These and other feats are carefully described in Chamber's *Edinburg Journal* (1).

The third case, which seems to parallel that of Signora Girardelli, is that of Nathan Coker (3) of Talbot County, Maryland. He, too, would heat a shovel to redness and place the hot object against his bare feet. Likewise, Coker would lick the shovel with his tongue until it cooled. He also claimed to be able to pick a red-hot piece of iron out of a forge, holding it in his bare hand without being burned. As far as is known, however, he did not do this last in the presence of examining physicians. The account of Coker's feats appears in *The Spiritual Magazine* for January 1872.

During the last half of the nineteenth century we encounter the so-called spiritualistic mediums, and although a great many of them were exposed as frauds, the most successful, Daniel Dunglas Home, was never proved to be so. In fact, his achievements (3) are so well documented by scientists and laymen alike that we must accept the physical phenomena for consideration, however much we are inclined to label his dealings with "spirits" as nonsense.

Home developed his ability to withstand heat at a rather late date in his mediumship, which, perhaps, accounts for his not carrying it further than he did. While appearing at the homes of the prominent people of his time,

he would horrify them by going to the fire-place, reaching in and taking a lump of red, glowing coals about the size of an orange and walking around with it in his hands. How it chilled their blood to see it! Try as they would, they could find no trickery. Home's hands were just like anyone else's.

It is possible that the trances that Home so laboriously appeared to induce before going through his repertoire with fire and hot coals were just so much acting, intended to mystify his audience even more. However, they may have served another purpose, that of allowing him to "screw up his courage" before carrying out his acts. We find some attempt at inducing a trance-like state in most fire feats.

The reports of various witnesses to Home's behavior are of considerable interest. Here is an account by Lord Adare of a seance that occurred on November 30, 1868:

Home, entranced, went back to the fire, and with his hands stirred the embers into a flame; then kneeling down, he placed his face right among the burning coals, moving it about as though bathing it in water. Then, getting up, he held his finger for some time in the flame of the candle. Presently he took the same lump of coal he had previously handled and came over to us, blowing upon it to make it brighter. He then walked slowly round the table, and said, "I want to see which of you will be the best subject. Ah! Adare will be the easiest because he has been most with Dan (i.e., himself)." Mr. Jencken held out his hand, saying, "Put it in mine." Home said, "No, touch it and see"; he touched it with the tip of his finger and burnt himself. Home then held it within four or five inches of Mr. Sarl's and Mr. Hart's hands, and they could not endure the heat. He came to me and said, "Now, if you are not afraid, hold out your hand." I did so and having made two rapid passes over my hand, he placed the coal in it. I must have held it for half a minute, long enough to have burned my hand fearfully; the coal felt scarcely warm . . .

Stanton Moses' account of Home's behavior is also remarkable. He writes:

Mr. Home went to the fire-place, removed the guard, and sat down on the hearthrug. There he seemed to hold a conversation by signs with a spirit. He repeatedly bowed, and finally set to work to mesmerise his head again. He ruffled his bushy hair until it stood out like a mop, and then deliberately lay down and put his head in the bright wood fire. The hair was in the blaze, and must under ordinary circumstances have been singed off. His head was in the grate and his neck on a level with the top bar. This was repeated several times . . .

Sir William Crookes, one of the most eminent scientists of his day, attested to the genuineness of Home's behavior with fire. Because of his stand, which did not, of course, include an endorsement of Home's "spiritualistic" activities, Crookes lost the esteem of most of his contemporaries in science. But he was stubborn to the end, refusing to recant. Even the motto he chose for his coat-of-arms flung defiance in their teeth. In Latin it read: *Ubi Crux, Ibi Lux*, which

translated freely has a double meaning. The literal translation is: "Where the cross is, there is light." Or cryptically, making a play on the Latin word *Crux*: "Where Crookes is, there is light" (2).

In the present century, most fire phenomena occur among primitive peoples living in remote, relatively inaccessible regions. In remote sections of Bulgaria, for example, according to several reliable observers, the natives practice fire-walking, dancing barefooted for lengthy periods of time on glowing beds of charcoal. These Bulgarian "ecstatics," who probably borrowed their knowledge from the early Mohammedan dervishes, are called *Nastinarians*.

Fire-walking is also practiced in the Macedonian village of Aya Eleni in Greece. The event takes place annually, and although the practice is dated from the year 1297, it is believed to derive from the rites of Dionysos, dating back before Christ. On St. Constantine's and St. Helen's Day, a number of the peasants dance barefoot on vast beds of glowing charcoal. This is maintained for hours and is accompanied by a form of self-induced hypnosis and ecstasy. Here again, it is the author's belief that the only purpose served by hypnosis and ecstasy is to build up and sustain courage, or, perhaps, to hide the means of actually performing the feat. As the peasants dance on the fire they hold icons. We see here a possible source of knowledge for those saints desirous of practicing fire phenomena.

In the Algerian area of North Africa, a certain group performs fire-walks. This appears to be one of the dervish sects. The secret is passed on from father to son and kept in the family.

At the Dervish monastery of Tekeit El Biktashia in Egypt, dervishes have been seen to pick a glowing piece of charcoal from a brazier and pop it into their mouths without burning themselves.

The Polynesians are reported to walk barefoot on red-hot rocks. These rocks are at a much higher temperature than charcoal fire-beds; yet, the Polynesians walk on them for longer periods than do many of the fire-walkers. The rocks are volcanic in nature, consisting of large chunks of porous lava. Huge fires burn for about two days, with the rocks heaped upon them. Numerous travellers have witnessed the practice, which is a part of religious observances.

The yogis of India and Ceylon are particularly adept at fire-walking. In Thurston's book (3) one can find a very engrossing account of a mass fire-walk in Mysore, India, in which several hundred people were reported to have participated.

The most incomprehensible of all modern fire phenomena is related by John Dingle in one of his books on travel through China and Tibet. Dingle tells of seeing a yogi in Singapore heat iron sandals red hot in a furnace, strap them to his feet, and then walk across a temple courtyard, removing them at the end of the walk. When Dingle examined the soles of the yogi's feet, he found them unburned. But when he touched one of the sandals with his finger, he received a burn, the scar of which he bears to this day. Yet, Dingle swears that he was not hypnotized and that the event actually occurred.

Finally, I will mention the work of Harry Price, the famous English psychic investigator. Harry Price, who loved truth as any scientist does, actually made his own fire bed and had Kuda Bux, a native of India, walk it while he made tests. At the time of his first investigations in 1937, he was unable to obtain the prolonged fire-walkings allegedly found among many primitive peoples. He concluded that only about two steps were possible under the conditions he

had set up. Later, with further tests, using Ahmed Hussain of Cawnpore, a professional fire-walker, Price was able to do a little better, but not making the fire-walk himself, he was not in a position to adequately evaluate his findings. He correctly decided that as far as he had gone, there was no paranormal explanation necessary, but because others could prolong the walk, he correctly concluded "... but I still think there is something else."

PERSONAL ATTEMPTS AT FIRE-WALKING AND RELATED BEHAVIORS

The author has performed the following fire phenomena:

Touched red-hot iron with my fingers.

Touched red-hot iron with my tongue.

Touched molten iron with my tongue. (No sensation! Can't feel it!)

Bent red-hot steel bars by stamping them with my bare feet.

Run barefoot on red-hot iron.

Walked on red-hot rocks.

Plunged my fingers into molten lead, brass and iron.

Taken a small quantity of molten lead in my mouth and spat it out immediately. Once I allowed it to solidify in my mouth and almost was burned. Never try this.

Carried red-hot coals around in my hands.

Popped red-hot coals into my mouth.

Chewed charcoal off burning sticks. This is easy if done fast enough.

Walked on beds of red-hot coals, taking eight steps to cross a fourteen foot pit.

Placed my fingers, hands and feet in candle flames until covered with carbon black. (No burns! Not hot! Only warm!)

Held my face, hands and feet in fire for a short time.

In all of these behaviors the contacts with fire or the hot object were, of course, of brief duration. I have never wished to continue the contact to the threshold of pain.

A SCIENTIFIC EXPLANATION

Modern man can take but one logical attitude towards "miracles," namely, that a "miracle" is only an unknown or unexplained natural event. This should be made clear because we are slowly beginning to realize that many of the "miracles" of former times actually occurred, but that modern man has lost the ability to perform them.

Fire-walking and other fire phenomena will always remain a great mystery, even with all of the facts known. It must take its place, however, with all of the other great "mysteries" known to science. What is more mysterious than a rainbow, the moon, the stars? If we had never seen any of these before, and then, were suddenly confronted with them, we would be struck dumb with awe. Seeing them constantly, they seem to lose this awesomeness. Yet, their inherent mystery remains. Everyone fears the unknown. I will never forget the fear that shook me, the first time I decided to touch red-hot iron with my finger. I had made enough tests and read enough on the subject to be certain that I could do it, and yet, when I came to try it, I shrank back in terror. For a moment I thought that perhaps I had lost my reason after all.

Time and again I attempted the act; and each time I couldn't. It is one thing to try a behavior like this with no one to guide you, and quite another to be shown how. The belief that red-hot iron would burn me was so ingrained that it required an almost superhuman effort of will to overcome it. Each new phase of the fire phenomena that I tried presented the same problem, with the act required appearing less difficult after it had been performed. My early fire experiences are treated rather thoroughly in the August 1957 issue of *True* magazine.

The fire-walk is a beautiful and awe-inspiring sight. When I first came to attempt a fire-walk on a red-hot bed of coals, I was faced with a fear even greater than when I had first touched red-hot iron. The reasons why I can perform the fire-walk and related behaviors are as follows:

First, there is natural moisture present on the surface of the skin. Under intense heat the skin sweats, and the moisture, which enters the spheroidal state, is converted to vapor, which occupies 22.4 molar volumes of the original moisture. This is why a microscopically thin layer of moisture can be a protection. We have then, skin, a layer of moisture in the spheroidal state, a layer of vapor, and the red-hot object.

The spheroidal state of a liquid can be described in the following example. When you drop water on a stove that is sufficiently hot, the liquid, instead of vaporizing in a flash of steam, rolls around for a long time on the stove and evaporates one-fiftieth as fast as it would at lower temperatures. If the stove is not hot enough the water immediately vaporizes in a flash as steam, and disappears. In the spheroidal state, then, the liquid is not actually in contact with the red-hot object. It is separated from it by a layer of vapor. The hotter the object, the longer the spheroidal state is maintained, and the greater the protection afforded by the cushion of vapor. Thus, it is safer to touch extremely hot objects than it is to touch only moderately hot ones. Brass, for example, which melts at 1706 degrees F., feels cooler to the touch in the molten state than does lead, which melts at 621 degrees F.

A second factor which helps to explain fire phenomena concerns the thickness of the skin. Longer contact with red-hot objects is possible with thicker skin. Some people have thick callouses on their feet and hands. This prevents the heat of the moisture under the boiling point of water from imparting a hot sensation to their feet or hands, and allows them to handle hot objects longer, as in the case of St. Francis of Paula. His hands must have been heavily calloused. I have duplicated this seemingly impossible feat by rolling the bar in my hands and, also, by bobbing it up and down, making intermittent contact with the hand. If there is a trace of fear present in doing this, you will falter and burn yourself. By rolling the metal, it doesn't remain in contact with any one spot too long.

In the instance where I walked on a large red-hot iron plate, the vapor in the spheroidal state held me above the iron plate, so that although the space was exceedingly small, there was no direct contact with the metal. This, by the way, so far as I know, was the first time the act had been done since the middle ages. The vapor held me up just as steam will push a piston up in a steam engine. The red-hot iron felt very slippery. This was the vapor cushion. You might say that I was walking on steam! Here, and in all cases, for the vapor cushioning principle to operate, the iron should be so hot that there will be no appreciable cooling when touching it. If the temperature drops too low, you will be burned.

In the case of walking on fire, there are additional factors to consider. With iron and molten metals, we are dealing with radiant heat alone. With red-hot coals, we have heat being generated by chemical action, i.e., oxidation. The spheroidal state hasn't too much opportunity to operate here; but, of course, it will where contact is made with the incandescent carbon, irregular as to surface. When the incandescent charcoal bed has burned sufficiently, a point will be reached when all the wood is burned out and practically pure carbon in the form of charcoal remains. It is fiercely hot, so hot that you can't approach the inferno without shielding your face. Even then, the heat drives you back. Iron thrust into it would be heated red hot.

At this point, if air is excluded from the carbon, it goes out instantly. The heat comes from the actual process of burning. When you step on the coals in this condition, the oxygen is cut off at the point of contact and the glow is momentarily extinguished. This is a curious thing. Here, a different action, together with the insulating properties of the skin and the spheroidal state, enables one to walk on fire. When a red-hot coal is brought in contact with moisture, the moisture is absorbed into the coal in an instant and extinguishes the glow at that point. The coal will not burn again as long as the moisture remains in it. Try to light a wet piece of charcoal. It can't be done.

The dervish who horrifies his audience by picking up a red-hot coal and putting it in his mouth is really not doing anything dangerous at all. Where it contacts his tongue, there is enough moisture present to extinguish the glow at the point of contact. He can still remove it from his mouth with the upper part still glowing. I have tried it several times and have never been burnt yet.

The chewing of red-hot glass by Richardson, the fire-eater, is unquestionably true. I have touched red-hot glass with my finger and tongue safely. He must have used thin wine glasses, and when he bit into them, being thin, they would have cooled enough with the saliva present, to shatter. He swallowed the glass, of course. Now here is a secret known only to eaters of drinking glasses, razor blades and light bulbs. One first eats a loaf of bread. The glass goes to the center of the mass as it churns about in the stomach, preventing any cuts. I learned this from one of the few who do eat glass. I would never try it myself.

In the case of Richardson cooking a small oyster on a piece of charcoal placed on his tongue, we must remember that the bottom of the charcoal would be wet with the saliva of the tongue. This won't burn. I could easily duplicate the feat. However, I would caution anyone against trying these experiments unaided. There are techniques to be observed, even when you know the principles involved.

And so we see that one group of "miracles" of ages past could actually have occurred. Now here is a strange fact. I find almost no embellishment in the fire phenomena I have related. Take, for example, the case in which the girl fell into the fire in a trance and was not burned. This is easily done. Let us assume that she fell just before the other party entered the room. The embers were almost burned out, yet the glow remained. When she fell, they were extinguished by the pressure of her body cutting off the air supply. It is really quite simple.

In the case of the yogi walking across a temple courtyard with a pair of red-hot iron sandals strapped to his feet, the walking produced an inter-

mittent contact with the metal. It was the spheroidal condition assumed by the moisture on his feet that saved him.

The spheroidal condition also operates with liquid against liquid. I have taken hot bacon fat into my mouth and expectorated it into a flame, causing the grease to flare up. This act and that using molten lead were used by Signora Girardelli in her presentations. She also took nitric acid into her mouth. This I have also duplicated, knowing that concentrated nitric acid is not as ionized as dilute acid and will not burn if the act is done quickly. Here again, the spheroidal state protected me. The nitric acid tasted like lemon juice and set my teeth on edge only slightly. There was about as much effect as in biting into a lemon, perhaps less. Of course, I spit the acid out quickly.

I have also plunged my hand into boiling oil and boiling lye, two acts that were attributed to St. Francis of Paula and considered "miracles." Never try these. They are dangerous if not done properly.

My purpose in performing these behaviors was to prove that stories about them in the past could very easily be true. I have always been very cautious in my approach to each of these phenomena and have never been burned, except in the case where I tried to walk on fire for the first time. The conditions were not just right, and one foot was faintly burned. The burn cleared up after a few minutes and I was able to make the walk safely.

It would be highly dangerous for anyone to attempt these behaviors if their nerves did not respond to heat stimulation. Such a person would have no warning when to stop the acts and would prolong them until burned.

SUMMARY

No paranormal explanation is necessary for fire-walking and related behaviors. With incandescent objects, protection is afforded by the spheroidal condition assumed by liquids. With glowing coals, a combination of the spheroidal condition, cutting off of oxygen, liquid absorption, and skin thickness operates to prevent a person from being burned.

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THE ISRAELI KIBBUTZ (COLLECTIVE SETTLEMENT) AS A "LABORATORY" FOR TESTING PSYCHODYNAMIC HYPOTHESES

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INTRODUCTION

As a result of minute observation and investigation of single cases, psychoanalysis evolved as a comprehensive theory of personality and its dynamics. Subsequent modifications and elaborations developed into the general field somewhat loosely entitled "psychodynamics." Its main foundation consists of a set of hypotheses, in varying stages of confirmation, regarding the roots of human behavior and the origins of personality and its development.

The effects of early infantile and childhood experience upon later personality development and behavior are of cardinal concern to the field of psychodynamics. Experimentation in this area is particularly difficult, if not forbidding, and mainly for two important reasons:

1. The life-span of human beings, the subjects to be investigated along the temporal dimension (effects of early experience upon later personality development), imposes serious limitations on longitudinal studies to be performed by experimenters with a similar life span.

2. For a variety of ethical reasons, the conditions and significant experiences of infants and young children cannot be manipulated extensively for experimental purposes. Under such restrictions, not all of the laboratory methods and controls can be applied in order to elicit scientifically valid results and conclusions about the effects of early experiences on later personality development.

For those who are still concerned with this issue, there remains an important avenue in which they may direct their efforts. This avenue has been followed rewardingly by several scientific disciplines. We refer to the investigation of persons reared in different cultures and societies, in which child rearing practices, family structure and intra-familial relationships differ markedly from our own. Also, some sub-cultural differences in these matters *within* a society serve the investigator in providing a natural setting for his research.

When such conditions prevail, the particular society itself becomes the "laboratory" where the important relationships, alluded to above, may be investigated. Usually, the "laboratory" society provides the "experimental" subjects, whereas another society may, sometimes implicitly, supply the "controls." Occasionally, there are sufficient variations in child-rearing practices within the same country and society so that significant issues can be investigated solely within its confines.

¹The author spent his sabbatical leave (calendar year of 1955) in Israel studying children in the Kibbutz setting.

Such fortunate circumstances are found in the State of Israel. There, one may find two sharply contrasted social structures and family organizations. One is very similar to the typical family found in Western society; the other, the Kibbutz, which is the subject of our examination, differs markedly from the former. Those differences may shed some light upon the psychodynamic issues under consideration.

THE SOCIAL SETTING

In the present section we shall concern ourselves primarily with the "different" social setting in which the child grows up in the Kibbutz — the collective settlement. Since the broader social and economic aspects of the Kibbutz society have been described elsewhere (4, 7, 8, 9) and would be beyond the scope of this paper, we shall concern ourselves, chiefly, with aspects of family organization and early interpersonal influences as they impinge upon the growing infant and child.

The areas related to child-rearing in which there are major differences between the Kibbutz and non-Kibbutz society may be classified as follows:

1. The pattern of physical arrangement.
2. Significant figures and socializing agents.

The Kibbutz infant begins his life, at the age of a few days, in a communal setting. Upon his return from the hospital with his mother, a few days after birth, he is placed with several of his peers in one of the rooms of the "infant house." With about three other infants, he is cared for and supervised by an infant nurse ("metapeleth"), who takes care of most of the physical needs of the group. The biological mother usually feeds her child, but is not at the infant house between feeding periods. When very young, the infant is visited afternoons by other members of his biological family. As he grows older (a few months) he is taken out for daily visits with the family, which last approximately from one to two hours. As the child grows older these periods are somewhat extended.

It is a rare occasion and exceptional circumstance (e.g., severe illness) which bring about the child's staying overnight with his parents. The rule is that he sleeps in his "own" house — the infant house, and later, the children's house, in the company of his peers and, usually, in the same room with one or more of them.

It may be postulated that in early infancy there are two "significant" figures of major importance in the life of the child — the biological mother and the metapeleth (2). Although the mother feeds the infant during most of the first year of life, and frequently plays with him, it is the metapeleth who spends more time with him, despite the fact that her time and attention are shared with three other children in the group.

When the child reaches the toddler stage and his social horizon broadens, other significant figures may be added and considered. The contacts with the mother are reduced, since she no longer feeds him, but the significance of the peers increases considerably. As the child matures, the peer group enters the field of his awareness much more prominently. He has to share with his peer group and work out a *modus vivendi* with it. He must respect their rights as well as stand up for his own. The group exercises control and may be a source of frustration. The group, and its individual members, is an important socializing agent for the egocentric toddler. Thus, much socialization begins early, via group living.

In addition, the father, whom he now visits more frequently, and the other biological siblings with whom he comes in contact during the "visits" in the family circle, become important figures in the life of the child. The primary socializing agent remains the metapeleth; but, the peer group, which takes over the standards mediated by her, is a further important extension of her authority. Later on, of course, beyond the toddler stage, such figures as teachers and other adults and children in the Kibbutz may become quite important to the child. Their importance in this "Gemeinschaft," due to their geographical proximity and to their concern for all the children of the Kibbutz, who are referred to by all the adults as "our" children, may be quite considerable.

HYPOTHESES

Considering the brief description of the child-rearing circumstances in the Kibbutz, as compared with the more conventional family setting, a series of hypotheses which emanate from psychodynamic and social-psychological theory may be posited and tested. The hypotheses, to be enumerated below, do not form an exhaustive list; they are among the more obviously testable ones. No attempt will be made to deal with methodological problems in the present exposition.

One series of hypotheses may be postulated from a consideration of the work concerning "maternal deprivation" and its effects on later personality development. Bowlby (1), in his review of the work of Goldfarb, Ribble, Spitz, and others, came to the conclusion that consistent and continuous mothering by one mother or maternal surrogate in the early years of life is essential for later mental health. The bulk of the information on which this conclusion is based stems from studies of separation of children from their mothers over varying periods of time and of children who were reared in the impersonal atmosphere of institutions. Under such conditions, early intellectual retardation, impulsivity and a host of other psychopathological symptoms are the characteristic findings. Since the Kibbutz child-rearing conditions may be characterized as "partial" maternal deprivation, due to "intermittent" mothering (6), the following hypotheses may be considered.

A. *Infants reared under Kibbutz conditions are more retarded in their intellectual development than controls reared under usual family conditions.*

B. *Children reared in the Kibbutz are more impulsive, more immature emotionally and less developed intellectually than children reared in the ordinary family setting.*

Recent studies (5, 6) in which relevant material was reported addressed themselves to these hypotheses. The findings led to a confirmation of Hypothesis A (6) and to a rejection of Hypothesis B (5). The caution that needs to be exercised in predicting from infancy to later childhood must be pointed out.

A more extensive series of hypotheses may be derived from the psychoanalytic theory of psychosexual development (3). Since the biological mother is not the only person upon whom the earliest object relationship centers in the Kibbutz-reared infant, the intensity of attachment to the mother may be assumed to be not as great as in children reared in the nuclear family setting. If this premise is acceptable, then a number of implications for later psychosexual development in boys and girls, consistent with psychoanalytic theory, may be seen. The Oedipal complex in boys may not be so intense, and castration anxiety attributed to a hostile father not so marked; consequently, the renunciation of mother as an excessively cathected object need not take place. The girls may not hold their mothers exclusively responsible for the lack of penis. The

metapeleth — the "co-mother" — may be considered the culprit. Consequently, the ambivalence of the girl to the biological mother may not be as great. Moreover, the transfer of the eroticism to the father may not be so intense either.

The foregoing discussion may have important implication for the identification process — the "forerunner of the superego." Since the father figure is not perceived as a strong castrating threat by the boys, the regressive identification with him need not necessarily take place, or at least, not universally. Identification may be more diffuse and extend to the feminine significant figures as well. If the father does not become the main object of the girl's love, then her identification with the mother figure is not so complete either. Here, too, the identification process may be assumed to be more diffuse than concentrated.

Another, more general possibility might be considered at this juncture — that of a less repressed relationship of the child to his parents. Since the parents are the main socializing agents in the early childhood of individuals who are reared in the ordinary family, they subject the child to frequent threat, frustration and control. This is a source of negative feelings on the part of the child toward the parental figures. Ambivalence develops. However, these feelings must be repressed, especially in view of later superego development, or else, loss of self-esteem may result. In the Kibbutz family such ambivalent feelings toward the parents may be fewer in view of the fact that the metapeleth bears the brunt of the socializing process. Moreover, these feelings may be more readily expressed with impunity, since parental deprivation or threat of loss of supplies cannot be so effective in this situation. Consequently, it may be anticipated that the expression of hostility, when felt, on the part of the Kibbutz child may be more open and closer to consciousness.

Consonant with the summary of the Kibbutz child's familial relationships from the psychoanalytic point of view, which appears above, a number of propositions in the nature of hypotheses may be stated:

C. *Kibbutz-reared individuals will give less evidence of strong, but repressed, attachment to the opposite-sexed parent than persons reared in the conventional family setting.*

D. *Kibbutz-reared individuals will evidence identification with the like-sexed parent less consistently than will controls.*

E. *Kibbutz-reared individuals will be freer and less repressed in the expression of hostility and ambivalence toward parental figures than will controls reared in the conventional family constellation.*

Another issue, concerning which predictions may be made, is that of sibling rivalry. The specific hypothesis to be stated takes into account two major factors which are embedded in the different intrafamilial relationships. In the first place, the child who, from his earliest infancy, is reared in the company of "competitors" is in a different position than the child in the ordinary family setting who finds well-established competitors upon birth or faces them upon their arrival after he has worked out a *modus vivendi* in the bosom of his family. Secondly, the ambivalent (and repressed) attitude toward the parents in the usual family setting is frequently displaced onto the siblings, against whom the expression of hostility is less subject to taboo. Since the expression of negative feelings toward parental figures may be less repressed in Kibbutz-reared children, the need for such displacement onto the siblings may be markedly reduced. If this reasoning is correct, then we may hypothesize:

F. Kibbutz-reared children will show less evidence of sibling rivalry than children reared in the conventional family.

In the foregoing paragraphs we have set forth several general hypotheses which are derived from psychoanalytic theory and may be tested in the Kibbutz "laboratory." A number of more specific predictions, emanating from these hypotheses, may also be made but are beyond the confines of our presentation. The list given above by no means exhausts all possibilities. Moreover, social psychological problems, as well as issues in group dynamics, may be readily examined in the setting of the Kibbutz which we have briefly described.

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THE USE OF PSYCHOMETRIC SIGN TESTS IN DIFFERENT POPULATIONS

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All too often the practical implications of a general theoretical article go unnoted for long periods of time. The reaction to the Meehl-Rosen antecedent probabilities article (1) seems to the author to be an important current example of this. The primary thesis of the Meehl-Rosen paper is that in the evaluation of psychometric signs or cutting scores consideration of validity (discriminating power) is not enough. Adequate evaluation of a psychometric device, it is argued, must consider the validity of the device in relation to the distribution of the criterion variable (base rate) in the population in which it is used.

Although the Meehl-Rosen thesis appears to have been generally accepted in principle, its everyday practical importance may not be as widely realized. It is the author's experience that while psychometric diagnosticians recognize that a change in the population split of a criterion variable can change the effectiveness of a psychometric device, they tend to regard these changes as rare events and frequently ignore the matter of base rates in usual assessment work. The primary purpose of this brief paper is to point out the marked changes in criterion population splits and consequent changes in test sign "meanings" which almost invariably occur in the application of clinical test signs in normal or abnormal populations.

Let us consider the rather common example of routine personality testing done in a college population. Again and again, students from these "normal" populations are saddled with various labels denoting severe emotional problems because the configuration of their scores on psychometric devices has been one which research has indicated is highly suggestive of, or almost invariably associated with, the labeled types of emotional disorder. When, as usually happens, further observation and inquiry yield none of the extra-test correlates of a labeled disorder, the diagnosis is questioned. Far too often the reply is: "That just shows that the trouble is deep; after all, research has demonstrated that when this sign is found the odds are three to one that the subject is schizophrenic."

The above is an example of applying research probabilities without considering base rate changes from the research population to the population in which the testing is done. To see the fallacy in this extension of research probabilities to other populations, the usual research procedures which yield the test signs should be examined. Almost invariably, the procedure is as follows:

A group of previously diagnosed schizophrenics (assume an N of 100) and a group of "normals" (N also 100) are tested or observed. This usual equating or near equating of sample size in sign validating research yields an artificial base rate of 50 per cent. The test results or behavior observations which show statistically significant differences between the two groups are then taken as "signs" of schizophrenia or normalcy (depending on which group showed the largest percentage of each sign). For example, suppose that in testing, 60% of the schizophrenics show a certain configuration of test responses while only

20% of normals have that configuration of results. This difference in percentage is statistically significant ($P < .001$) and, extending the ratio of obtained diagnostic group percentages, it is said that whenever this configuration of test results arises, a diagnosis of schizophrenia will be right three times as often as it is wrong. (For the sake of this example let us make the case especially easy by assuming that no other diagnostic groups, such as neurotics or brain-injured, exhibit this response configuration in psychological testing.) Actually, this type of reasoning approximates the correct probabilities of accuracy of diagnosis only in the rather rare situation where the number of persons in the various diagnostic groupings which yield the psychometric sign are proportionately distributed in the research sample and in the population in which the diagnostic work is carried out.

From the example reported above, it is correct, that if we can assume research has been done on a random sample of schizophrenics and normals, the best estimate of the percentage of schizophrenics and normals which would show the sign is the percentage derived from the research samples. However, it does not necessarily follow that because the sign percentage was three times as great among the schizophrenics as normals, that this sign should be used as an indicator of schizophrenia. A little arithmetic will show why.

Consider the case of any relatively random sample from the general population. In this population we could estimate that there are roughly one million schizophrenics and about 150 million normals (a base rate of two-thirds of one per cent compared to the usual research base rate of 50 per cent). Extending our previously assumed diagnostic sign rates for the two populations, we can quickly compute that in the general population about 600 thousand schizophrenics (60 per cent of one million) and approximately 30 million normals (20 per cent of 150 million) would exhibit this sign. In other words, whenever we see this "schizophrenic" sign in the general population we will be wrong fifty times for each time we are right, if we always attach the label "schizophrenic" to anyone who shows this sign (the ratio of 30 million to 600 thousand). On the other hand, in working in a hospital population this test configuration would be a far better indicator of schizophrenia than the research results show. We have said that this diagnostic sign is shown only by the schizophrenic and normal groups, and in the hospital population a "normal" is an extremely rare event. If we could say that there were no hospital "normals" diagnostically tested by the psychologist, then, without error, we could attach the label "schizophrenic" to every patient whose test results showed this diagnostic sign. Because of the uniformity of test sign validating research procedures and because of the consistently extreme base rate splits of the various clinical groupings in both normal and hospital populations, this example has general significance.

In summary, the validity of diagnostic signs should not be evaluated solely by tests of the statistical significance of differences in proportions of persons exhibiting a sign in different diagnostic groups. Instead, it must be realized that the usefulness of a diagnostic sign is a function both of these group proportional differences in producing the sign and of the proportional sizes of the various diagnostic groups in the population being tested. These considerations are of especially great importance in the diagnosis of abnormalities, since the base rates of abnormalities in both "normal" and hospital populations are markedly different from the usual 50 per cent research base rate.

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PERSPECTIVES IN PSYCHOLOGY

IV. ON THE VALIDITY OF THE EXPERIMENTAL APPROACH TO BEHAVIOR

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Academic psychology is an *experimental* psychology. Its faith is in scientific method, its motto: "The path of understanding lies through the laboratory." For almost 80 years now, since Wilhelm Wundt in 1879 founded the first laboratory of psychology, this motto has steadily grown in importance as a guide to the training and professional activities of psychologists. To many students of behavior, laboratory research is the highest and most constructive activity to which a psychologist can aspire. It is intrinsically laudable. Like the "magical" performances of primitive peoples in Polynesia and Australasia, which the natives accept as physical manifestations of mana, the doing of laboratory work earns for the psychologist a special distinction, which makes invidious any comparison between him and his non-experimentalist colleague. Under the influence of this attitude the laboratory becomes a temple. Faith in experiment becomes a creed. In the words of the apostle: *Ab Experimento Omnia Disce*. "From Experiment Learn All."

This description of modern academic psychology, with its obvious aspects of caricature, needs to be qualified. Most psychologists temper their enthusiasm for the experimental approach with a healthy respect for the complexity of their subject matter. Seldom, however, is the *validity* of the experimental approach to behavior questioned. Although much human activity does not at present easily lend itself to experimental analysis, few psychologists would doubt that given sufficient time and effort even the most complex behavior will be amenable to experimental procedure. It is this unqualified faith in the validity of the experimental approach that makes of the preceding description less of a caricature than it might otherwise be. Since, as Montaigne remarked, "Nothing is so firmly believed as what we least know," a skeptic's criticism of this faith might not be without value. At the very least, it may generate some controversy; and psychology, which shows definite signs of settling into a bed of Procrustes, can certainly benefit from that.

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What are the requirements of a valid experimental psychology? They would appear to be two in number. First, experimental procedure must make possible rigorous quantitative and qualitative analysis. Second, subjects must be permitted sufficient latitude of response to approximate real life conditions of behavior.

The perennial problem of experimental psychology is to reconcile these two needs. In the recent past, attempts to cope with this problem have been strongly prejudiced by an almost Pythagorean cultivation of quantitative techniques, unleavened by a compensating concern for the significance of the behavior being studied. This intense but misguided effort, in which observation

is equated with measurement and knowledge with numbers, finds a rough but suggestive parallel in the obsessive concern with form that characterizes the "scholastic" school of modern literary criticism. Of these "new" critical schoolmen," Van Wyck Brooks (1) has written:

. . . When, in connection with criticism, one hears too much of "symbolic textures," not to speak of "categorical expectation," one feels that criticism itself has followed the path of scholasticism and has travelled too far from its source in literature. These are the exegetical questions that scholars have always relished, the games that learned men love to play but that lead to the "deserts of ingenuity" that Mark Van Doren has characterized in this "faulty science at best" that is "not an art."

Bold words from Mr. Brooks, which have a special relevance for modern psychology. We, too, have our "deserts of ingenuity," in what is at best a "faulty science." Just as the critic may break his contact with literature by focussing upon form and ignoring content, so the experimental psychologist may unwittingly abandon his search for the principles of behavior by an overconcern with method to the neglect of subject matter. The result is a rigorous quantitative analysis of laboratory artifacts.

Preoccupation with method among mature psychologists is a potent determinant of undergraduate and graduate curricula. Thus, some universities offer a lower division (freshman-sophomore level) introductory course in experimental psychology, in addition to the more traditional upper division (junior-senior level) laboratory work. Just what benefit a first year student can derive from this training is not at all clear. It can be argued, of course, that if the student acquires a rudimentary knowledge of scientific method and of the necessity for controlled observation, a worthy objective has been achieved. But the effect may not be entirely salutary. In the first place, many of our most valuable insights into the nature of human conduct have resulted not from the controlled observations of professionally trained psychologists but from uncontrolled, yet highly perceptive observations of "amateur" students of behavior—novelists, poets, social critics, etc. Who would dispute, for example, the almost clinical insights of Shakespeare, Dostoevsky, Tolstoy, Dickens, and other literary giants? A reasonably thorough acquaintance with this store of knowledge and a cultivated appreciation of the productive potential of the "untrained" but perspicacious student of humanity would seem to be basic to an intelligent understanding of the utility of scientific procedure in psychology. The typical first or second year candidate cannot claim either of these attributes. Possibly, he will never attain to this level of understanding. In any event, too early exposure to laboratory procedure as the "preferred" way of observing behavior is all too likely to result in a negative, disparaging attitude towards the "untutored" observations of the perceptive "amateur." This is scientism at its very worst.

Of necessity, the subject matter for first year laboratory study must be very simple and artificial in nature, e.g., mirror tracing, finger maze learning, auditory reaction time, word association, etc. More complex behaviors would present problems of control far beyond the competence of the beginning student. This suggests a second disadvantage inherent in too early exposure to laboratory procedure. Mirror tracing, finger maze learning, word association, and similar laboratory activities have a dubious significance when compared with the indescribable richness and variety of real life behavior. Yet, the neat-

ness and precision with which these simple behaviors can be objectively studied may persuade the student that in psychological work control is primary and significance of subject matter, secondary. With this bias the professional psychologist turns his back on the complex, but very real behavior of everyday life in favor of laboratory fictions, i.e., well-controlled but highly abstract, artificial activities. Conceivably, this prejudice may not form so readily if experimental training is delayed until the student is intellectually equipped to distinguish real behavior from laboratory artifact and to appreciate the advantages of control without being blinded by them.

Perhaps the most flagrant example of psychological investigation of highly abstract, artificial activities is to be found in the work of Gustav Fechner. Recall, here, the devastating critique of Fechner's research by William James, which appears in the distinguished philosopher-psychologist's *Principles of Psychology* (2).

. . . The Fechnerian *Maasformel* and the conception of it as an ultimate "psychophysic law" will remain an "idol of the den," if ever there was one. Fechner himself indeed was a German *Gelehrter* of the ideal type, at once simple and shrewd, a mystic and an experimentalist, homely and daring, and as loyal to facts as to his theories. But it would be terrible if even such a dear old man as this could saddle our Science forever with his patient whimsies, and, in a world so full of more nutritious objects of attention, compel all future students to plough through the difficulties, not only of his own works, but of the still drier ones written in his refutation. Those who desire this dreadful literature can find it; it has a "disciplinary value;" but I will not even enumerate it in a footnote. The only amusing part of it is that Fechner's critics should always feel bound, after smiting his theories hip and thigh and leaving not a stick of them standing, to wind up by saying that nevertheless to him belongs the *imperishable glory* of first formulating them and thereby turning psychology into an exact science (1).

"'And everybody praised the duke
Who this great fight did win.'
'But what good came of it at last?'
Quoth little Peterkin.
'Why, that I cannot tell,' said he,
'But 'twas a famous victory!'"

Laboratory work in psychology has a powerful appeal for particular types of personalities. Others, more philosophically or humanistically inclined, regard this approach as too restricted, too artificial to satisfy the needs of the student of behavior. These individuals prefer an *observational* approach, which will permit the study of behavior as it is actually lived in day to day experience. They accept the insights of the perceptive "amateur" as valid psychological data. What effect does a first year laboratory course have on students of this orientation? Many of them, and this includes the ablest of the lot, are discouraged from taking further formal work in psychology. Their mistress is not the laboratory of the psychologist but the laboratory of real life. Finding little encouragement in their early psychological work for an idiosyncratic approach to behavior, these students leave the field and seek out other, more permissive areas of study. In this way a number of very highly gifted individuals are lost to the profession.

The experimental foundations of modern academic psychology need to be reexamined and the question of the validity of the experimental approach carefully explored. As a small contribution to this professional self-inspection we offer the following general observations.

All behavior involves the integrated action of many different factors. Some of these are biological in nature — for example, the nervous system, the circulatory system, the glandular system, the muscular system, the digestive system, etc. These organ systems make an important contribution to behavior. Knowledge of their structure and function is indispensable in psychological work. We anticipate no argument when we suggest that this knowledge can only be gathered through the experimental approach. Although biological action is not completely independent of cultural influence, the relatively invariant operation of structure-function mechanisms makes them peculiarly susceptible to rigorous laboratory analysis.

The study of structure-function mechanisms is actually physiological work. When we investigate behavior, on the other hand, we are concerned with a whole organism, having a particular experiential history, functioning in a particular setting through interacting with a particular stimulus object. Nothing less than this is the subject matter which the experimentalist must submit to rigorous quantitative and qualitative analysis. Can this need be met in laboratory situations which allow the organism sufficient freedom of response to approximate real life conditions of behavior?

No unqualified answer can be given to this question. Simple behaviors, activities in which physiological processes predominate, behaviors coordinated with minimal stimulation and stimulus deprivation — with these forms of action a valid experimental psychology seems not only possible but distinctly probable. But what of the more complex behaviors, in which conduct is coordinated with very complex patterns of stimulation and in which the factors of past behavior history and social conditioning are of paramount importance? What is the outlook here? Do present laboratory techniques show promise of providing us with the means for ultimate understanding of these behaviors? Can we, for example, by studying the reactions of a subject peeking monocularly into a distorted room or watching the "movement" of a stationary light in a darkened room, arrive at a genuine understanding of the complex perceptual processes of everyday life? Possibly we can. Certainly, we should not cease trying. We must not give up experimenting with complex behaviors. But it might also prove true that the really significant "break-throughs" in understanding complex behaviors will result from the perceptive observations of the non-experimentalist "amateur" and non-experimentalist professional. Is it not in this connection, for example, that the genius of a man such as Freud is most intelligently understood? Freud's observations of behavior in the clinical situation have yielded a wealth of material for further study unmatched by the contributions of any other psychologist in the modern period.

These remarks on the validity of an experimental approach to behavior are simply tentative suggestions. The problem is much too complex to be adequately treated by a single individual, and we have made no attempt to do so. But perhaps other psychologists who disagree with the observations made here will be stimulated to express their own views on the subject. It is to be hoped that out of this expression a more intelligent understanding of the problems of studying human behavior will emerge.

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FURTHER COMMENTS ON THE DEFINITION OF EMOTION

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Dr. Brackmann (1) has recently again raised the problem of how to distinguish emotion from non-emotion, and in general, how to define the term. He concludes that Kantor's definition, namely, "a definite type of failure to perform an expected form of adjustment or adaptation upon the basis of surrounding conditions and the individual's reactional biography or previous behavior history," is the only satisfactory definition extant. There is the implication in Dr. Brackmann's comments that the problem of definition has been one of the stumbling blocks to the adequate development of this area of psychology.

In a recent paper (2), I have noted, and given evidence for the belief, that the many difficulties and disagreements in the study of emotion are due to six major problems and not just one. These can be briefly described in the following terms:

The problem of intensity.—Emotions differ not only qualitatively but quantitatively as well. Different investigations, even when concerned with the same general problem, may study the same emotion under different degrees of arousal; there is evidence that different effects may result.

The problem of persistence.—Some studies deal with emotion as produced in the laboratory by loud noises, electric shocks, etc., while many others deal with clinical patients suffering anxiety, depression and the like, over a period of months or years. Evidence has recently accumulated to show that the chronic emotional state may differ markedly from the acute one, even for emotions given the same name.

The problem of purity.—Laboratory studies dealing with transient emotional states are more likely to be concerned with a relatively pure emotion. Clinical studies, on the other hand, are more likely to deal with mixed emotions whose properties may be quite different.

The problem of individual differences.—Recent observations have shown that when a group of subjects is exposed to the same general conditions of stress or other adaptive tasks, the variations in response can frequently be grouped into a few subclasses which have large consistencies within a subclass and marked differences between them. This implies that a simple average of the response measures for a group of subjects may be misleading.

The problem of introspection.—A number of current "theories" of emotion rely on introspective reports, or the feeling of an emotion as a central concept. Yet, the evidence for theories often comes from animal studies, or from clinical reports of patients who have repressed their feelings. If these kinds of evidence are to be admissible, then the subjective feeling of an emotion should not be a necessary part of the definition of an emotion.

The problem of definition.—In the light of these various problems it is suggested that a definition of emotion be such that it (a) does not require (although it may use) introspections, (b) can be applied to lower animals and patients with "unconscious" emotions, and (c) can be applied to mixed, pure,

or varying intensities of emotion. This implies that there are overt as well as covert patterns of expression which individuals show to various kinds of situations. These total body reactions, of various magnitude and pattern, to certain types of situations are what we generally mean by the term emotion. The actual determination of these patterns is an empirical task which can be approached gradually by a series of successive approximations.

There are two other important implications of this analysis. First, the idea of distinguishing between emotion and non-emotion is in a sense a pseudo problem; there is no sharp line of demarcation between total body reactions called emotions and similar reactions stemming from other causes, such as exercise or illness, particularly if they exist to slight degree. Secondly, a definition of emotion which emphasizes "failure to perform," or "disorganization," can only relate to a limited aspect of the broad spectrum of emotion. Joy, pleasure, surprise, and curiosity, are as important emotions to be understood in the framework of a general theory as fear and anger.

It seems to the present writer that an adequate theory of emotion must deal with all of the problems raised here.

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